

[x].. O.J. Lewis. J. McClintock.

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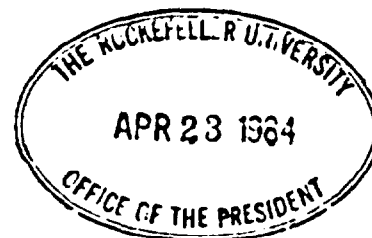
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April 18, 1984

Dr. Joshua Lederberg, President
The Rockefeller University
1230 York Avenue
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Dear Joshua:

I hadn't seen the Gould review, but did read some others and the book by Evelyn Keller. I have discussed it with Alex several times, and lent him my copy of Keller's book. She quotes Barbara as saying that she couldn't communicate with Alex because they were "on different levels of thinking". Alex says that he has been told that she says he didn't understand her work. He complains she regards those who disagreed with all her interpretations as misunderstanding, rather than understanding but remaining unconvinced. Of course, he remains an admirer of her work.

To me, as to Alex I am sure, the difficult line to follow is to be admiring of Barbara's extraordinary observational and analytical skills and and at the same time downplay the aura of the prophet crying in the wilderness. The parts of her interpretations that were controversial in the 1950s are still controversial. I believe it was Fincham in his review in Nature who said that the jury is still out.

I am not surprised at the Gould review. He enjoys being a revisionist. Barbara, as the unorthodox, the outsider, the unrecognized genius fits his pattern. Her holistic views would certainly appeal to him, as would her views of the "plasticity" of the genome; never mind the conservation of linkages.

Were you here when Barbara gave a seminar and got into an argument with Sewall? It was around 1956. I don't remember your being there; if you were, your memory is probably better than mine. In any case there was considerable erasing of each other's diagrams from the blackboard. Sewall tried to get her to fill in a matrix classifying the properties of her elements, which she refused to do. According to Alex, she was exceedingly annoyed at Sewall's persistent questioning and this is the reason she has never come back to Madison. If she is waiting for Sewall to die, she chose the wrong person.

She certainly was not an unknown figure. I was well aware of her eminence when I was a graduate student at Texas in the late 1930s. I was not at the 1951 Cold Spring Harbor Symposium, but I do remember people commenting both her interpretations and her preoccupation with them. Of course no one questioned the skill and insight that she brought to her observations. I remember our discussing this point several times. The transposability of the elements was not questioned, as far as I know, but the role that they played as "controllers" was the sticky point.

Life for me is very interesting now. I am head over heels in P factors, vicariously in the sense that I am doing essentially none of the lab work, but discuss it a great deal. Most of the experiments that I have done in the past have involved long times and the day to day work was uninteresting. It is fun to be involved in something where there are new findings all the time.

The letter you referred to was from Ann. I never got your possibly "too intimate" reply. The next time you write a letter and then think better of it, send it anyhow.

Your letter started Ann and me off on a round of sentimental, happy, affectionate memories. It's hard to come to grips with David's old enough to be at Harvard, but I bow to simple arithmetic.

All the best, from Ann and me.

Sincerely,

A handwritten signature in cursive script, appearing to read "Jim".

P.S. Do you remember Leah (Hiller) Lowenstein? She had been Dean at Jefferson Medical School and unexpectedly (to me, at least) died of cancer. Her husband, John, worked with Phil Cohen. She frequently played string quartets with us and was, I think, present on at least one occasion when you were there.